

**MODEL-BASED GUIDANCE FOR  
RESTORING CHINOOK SALMON: RIVER,  
ESTUARY, AND OCEAN INFLUENCES ON  
POPULATIONS SPAWNING IN THE  
SACRAMENTO AND SAN JOAQUIN BASINS**

**Yetta I Jager**

## **Public Comments**

No public comments were received for this proposal.

# Collaboration Panel Review

## Proposal Title

#0201: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AND OCEAN INFLUENCES ON POPULATIONS SPAWNING IN THE SACRAMENTO AND SAN JOAQUIN BASINS

Final Panel Rating
adequate

## Collaboration Panel (Primary) Review

### Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

**adequate**

Reference the statement in Section 2.2 (Tasks, page 10), "Many of these tasks could be conducted independently . . . ." The subtasks are intended to produce discrete products, and collaboration is involved in most efforts. The consequent end-products, however, are a of a disparate nature and are fairly independent of each another. The end-products will be directed at inclusion into an existing generalized model in an attempt to improve its reliability to address management issues and will also be available for independent uses.

### Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

## Collaboration Panel Review

adequate

The proposal includes a conceptual model of the salmon life cycle relationship to the environment (Section 1.3, page 8) and a timeline for program tasks (Section 2.3, pages 20-21.) The individual tasks and sub-tasks are well-defined. The text discusses (page 22) alternative strategies in case the results of previously-programmed work are not available.

### **Project Management:**

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

adequate

The Lead Investigator is prominently identified as the overall project leader. The text describes collaborative activities but they are not depicted in the Tasks form. The text identifies alternative strategies (page 22) if previously scheduled work is not available.

### **Team Composition:**

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

adequate

The text indicates the Lead Investigator has sufficient experience leading collaborative efforts but details are lacking regarding the nature of the responsibilities and of the composition of the collaborators. The degree of commitment is not explicitly defined. The various participants have individual responsibilities that are well-defined. The composition of the teams seems appropriate for the task assignments. Three personnel are listed as collaborator in sub-task 4.5 (Section 2.2, page 17) while the Task form (page 3) and the Budget form list only one person. There are numerous other discrepancies in the definition of individuals'

## Collaboration Panel Review

responsibilities included in Section 2.2 (pages 10-18) and the Tasks form. Consequently, these discrepancies confound the expectation of the work to be done, including the deliverables.

### Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

inadequate

Statements regarding the type and timing of interim and final reports, etc., are included in the Products sections of the sub-task discussions (Section 2.2, Tasks, pages 10-18) and elsewhere. However, this is not so much a clear plan as a reporting of the anticipated type and schedule of releases. There are no dedicated funds or separate tasks in the Budget form or the Tasks form that addresses communication of results.

### Additional Comments:

## Collaboration Panel (Discussion) Review

Primary reviewer judged that most tasks were to be completed by individuals in a given entity. He noted that the statement of "many of these tasks could be conducted independently" was a detractor in the proposal and felt the details in the proposal were not consistent throughout. The secondary reviewer concurred with the primary in all areas.

# Technical Synthesis Panel Review

## Proposal Title

#0201: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AND OCEAN INFLUENCES ON POPULATIONS SPAWNING IN THE SACRAMENTO AND SAN JOAQUIN BASINS

Final Panel Rating
adequate

## Technical Synthesis Panel (Primary) Review

### TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: The overall goal of this project was to reduce uncertainty in management of Central Valley chinook salmon. Specifically, the authors propose to develop a model to evaluate flow management in upper rivers, entrainment in lower rivers, and harvest in ocean. Specific focus of the proposal was to look at the interaction of temperature and flow as factors affecting Chinook survival. The authors will develop 3 models to evaluate factors affecting fall and spring salmon in two rivers, the Tuolumne and Yuba Rivers. They will revise an existing individual-based model developed for fall chinook salmon in the Tuolumne River to include spring and summer runs, and use it to model salmon in the Yuba River with habitat specific data. A separate model will be developed to model salmon in the delta and ocean habitats. The authors will include as input variables daily temperature and flow, mesoscale habitat data, spawner size and number, timing of migration, and smolt data. The authors will update existing input data for the Tuolumne River, and use available data from the Yuba River to simulate Chinook salmon in that system. The project is feasible, and the investigators are competent and experienced with modeling. The budget is high but reasonable considering the senior level of the authors.

#0201: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AN...

## **Additional Comments:**

Reviewer concerns were that the proposal was loosely organized and written, and details were lacking. Details of model structure were not available to understand how Chinook salmon were simulated of the IBM was not specified. Steelhead populations are considered for modeling almost as an afterthought. Why were these 2 rivers were chosen, except as a contrast in temperature regimes? Why was the habitat scale model habitat is mesoscale. Details of the delta and ocean survival model component is very vague. The proposal lacked integration among component modeling efforts (river vs delta vs ocean). It was not clear how information gained from the model would be made available to managers. No evaluation of quality or quantity of existing data was discussed. Reviewers felt that more effort should have been spent on collecting needed data. Also, they felt it would be useful to be able to compare model predictions with field studies. Validation and uncertainty analysis are essential components of any modeling effort, but are not adequately addressed in this proposal. Key issues and assumptions for the modeling approach should be presented. Task 4 (modeling Chinook salmon in the estuary and ocean) seems difficult, and might be cut from the proposal without significant loss of results.

Summary: The overall goal of this project was to reduce uncertainty in management of Central Valley chinook salmon. Specifically, the authors propose to develop a model to evaluate flow management in upper rivers, entrainment in lower rivers, and harvest in ocean. Specific focus of the proposal was to look at the interaction of temperature and flow as factors affecting Chinook survival. The authors will develop 3 models to evaluate factors affecting fall and spring salmon in two rivers, the Tuolumne and Yuba Rivers. They will revise an existing individual-based model developed for fall chinook salmon in the Tuolumne River to include spring and summer runs, and use it to model salmon in the Yuba River with habitat specific data. A separate model will be developed to model salmon in the delta and ocean habitats. The authors will include as input variables daily temperature and flow,

## Technical Synthesis Panel Review

mesoscale habitat data, spawner size and number, timing of migration, and smolt data. The authors will update existing input data for the Tuolumne River, and use available data from the Yuba River to simulate Chinook salmon in that system. The project is feasible, and the investigators are competent and experienced with modeling. The budget is high but reasonable considering the senior level of the authors.

## Technical Synthesis Panel (Discussion) Review

### TSP Observations, Findings And Recommendations:

The primary reviewer conveyed that that this is an adequate proposal. The secondary reviewer felt this was a good proposal, but that the model could be difficult to apply to the Delta System because of a lack of knowledge about Chinook behavior and habitat use in that area. Much data are currently available, but possibly not the type of data needed to address this issue. The authors did not offer a critical evaluation of existing data that might be used to parameterize the model. The investigators are competent and experienced.

The panel discussed whether the proposal is adequate, and noted that the existing model has gone through peer review. The authors must be careful with the execution of the model and include a better consideration of uncertainties. The model would provide useful information, but will need local people on the team to limit simplified assumptions and provide understanding of the data available. The panel agreed that the proposal contained serious deficiencies that needed to be corrected.

Final ranking: Adequate



# Technical Review #1

proposal title: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AND OCEAN INFLUENCES ON POPULATIONS SPAWNING IN THE SACRAMENTO AND SAN JOAQUIN BASINS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The stated goal of this proposal is to reduce uncertainty in management decisions (principally, those related to instream flow) for central valley chinook salmon and steelhead trout. To that end, the authors propose to further develop an existing individual-based model to evaluate (1) how optimal flow regime is affected by average stream temperature; (2) the impact of drought conditions on population persistence (3) the importance of estuarine / delta processes in affecting population dynamics.</p> <p>Accompanying these objectives are several related tasks, most of which pertain to gathering more detailed data on instream temperature conditions, and finally looking at existing data on salmon abundances to find empirical support for the model output.</p> <p>I fear that the authors are putting the cart before the horse here. Have they demonstrated the critical importance of early-life history events on population viability? Analyses of Columbia river salmon point to a very limited role of in-stream residence on population growth (e.g., Karieva et al. 2001; Science). Because this type of analysis is so simple to do (and so inexpensive), I don't understand why it hasn't done first to justify the detailed and complex model proposed here.</p>
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## Technical Review #1

Rating	fair
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### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>The proposal rests on the assumption that temperature is the dominant driver of chinook and steelhead recruitment, and that flow affects recruitment through its effect on instream temperature. I'm not convinced that this will hold true. As tempting as it is to believe that the laws of thermodynamics and physics are sufficient to predict fish recruitment, I have not come across any convincing evidence to support this assumption. That isn't to say it is unimportant, but I don't think that the authors have put forth a convincing justification for spending 1 million dollars on one assumption, without considering a larger suite of processes.</p> <p>After reading through the proposal a few times, I found myself asking the following questions:</p> <p>Have the models indicated that more detailed in-stream thermal data is crucial for decisions? (my suspicion is that this information is NOT crucial). If so, what is the evidence for this?</p> <p>Why is modeling needed for these questions? Modeling already has been useful in this system to identify some hypotheses. It seems to me that the next step is to see if those predictions are borne out. I would be much more enthusiastic if the empirical work proposed here (task 4.2) was the MAIN emphasis, rather than a side-project.</p> <p>Why are individual-based models appropriate? The justification presented was unconvincing. The</p>
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## Technical Review #1

	<p>overwhelming consensus of the professional community is that the models of intermediate complexity are optimal for decision analysis. The model proposed here is clearly over parameterized to be of use in a decision framework. The authors list several biological and ecological processes that can be represented explicitly in IBM's. Yet, most of these can easily be represented at a population scale through simple functions (i.e., integrating over some distribution to get a mean response).</p> <p>Do IBM's such as this have any track record in improving managment decisions?</p> <p>On what basis will the model performance be assessed? Predictive capacity?</p> <p>If the rationale for the model development is to improve decision making, then it should start with the decisions of interest, and build the model around them. It's not clear what management decisions the model will be developed around.</p>
<b>Rating</b>	poor

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

<b>Comments</b>	<p>The rigid adherence to one (very complicated) modeling approach is a major weakness of this proposal. I admit that I am a skeptic of these sorts of models, but that skepticism is borne out of the widespread appreciation that more complex and detailed models rarely lead to improved decisions. Don Ludwig has written some wonderful stuff on this, and Hilborn and Walters summarize these thoughts nicely in their 1992 book. There is a real danger in being seduced by the</p>
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## Technical Review #1

	complexity of these reductionist models. Yet this approach, which treats fish like little thermodynamic particles, misses all of the complexities that emerge through subtle behavioral and evolutionary processes. Much better to admit ignorance about these (and consider a variety of alternate structural forms of models) than to develop an overconfidence in one model. In fact, I don't recall ever seeing an IBM evaluated from the perspective of structural uncertainty.
<b>Rating</b>	fair

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?  
Is the scale of the project consistent with the objectives and within the grasp of authors?

<b>Comments</b>	I do think that the authors will have working models by the end of the study, and that a few academic publications will result. I'm most concerned about the source of parameter estimates, particularly for the work that aims to explore estuarine conditions. In fact, little information is given about what will be represented in that model.
<b>Rating</b>	fair

## Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

<b>Comments</b>	This proposal seeks to use much of the existing monitoring data, while adding some additional study on instream thermal conditions.
<b>Rating</b>	good

## Technical Review #1

### Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	I suspect that a very large, complex model will be produced, and that this may or may not produce some better management decisions.
Rating	fair

### Additional Comments

Comments
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### Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	I would feel much more comfortable if the authors were more broad-based in their modeling approaches and techniques. I'm reminded of the saying that "if your only tool is a hammer, all of your problems look like nails". That said, these authors know this particular model, and the ecological system quite well. They have the infrastructure to conduct this proposed work at their disposal.
Rating	good

### Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This is an extremely expensive proposal, especially given that no costly field work is involved. A more focussed study that looked at existing time series data (along the lines the W. Kimmerer has done with
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### Technical Review #1

	striped bass) would probably yield much more insight and a small fraction of the cost.
Rating	poor

## Overall

Provide a brief explanation of your summary rating.

Comments	As a quantitative ecologist, I object to any proposal that rigidly adheres to one type of model, especially one that seems to be based on the myth that large-complex, and expensive models are needed for decision making. Carl Walters and Tony Starfield have both written convincingly that these "myths" are counterproductive to useful development of models and the implementation of model analysis for decision making.
Rating	fair

# Technical Review #2

proposal title: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AND OCEAN INFLUENCES ON POPULATIONS SPAWNING IN THE SACRAMENTO AND SAN JOAQUIN BASINS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals of the study, as I read them, are the coordination of physical (flow and temperature) models and fish life history models to assess the areas of greatest loss, and potential gain from rehabilitation efforts. This makes good sense to me.
Rating	excellent

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	Perhaps it is "common knowledge" within CalFed and the region exactly what the status of the different runs of salmon and steelhead are in the Central Valley, but this information was not presented. This is not merely a matter of referring to all the listed ESUs but a real presentation of the things that are stressing them. Surely some information is available. This part of the proposal was very light, especially with respect to the fishes themselves and the specific rivers. Most of the papers cited are models, and many of them by the group submitting this proposal. I would have liked to have seen better grounding in solid
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## Technical Review #2

	salmon biology.
Rating	fair

### Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Modeling as a scientific technique is, as the authors point out, an important leg of the "research triangle", along with field and laboratory studies. Thus the development and application of models must go hand-in-hand with the other forms of scientific endeavor. Thus as an approach I cannot fault this proposal. However, the integration with the other forms of data is very casual. I am concerned by the lack of detail on the kinds of data that will be used (e.g., fish counts, survival rates, distribution, etc.). None of the lead investigators seems to have any background in salmon biology, and it is not evident that the importance of data is fully appreciated. I therefore am confident that the team will construct sophisticated models, but I am not at all confident that the models will tell us much that will address the problems they outline.
Rating	good

### Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The construction of the kinds of models that are described does not seem at all unfeasible, at least not to a team as experienced as this one. What is less clear is how useful the models will be. The
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## Technical Review #2

	availability of high quality data is simply not made clear at all. My reading of the literature on California salmon, and also my evaluation of other proposals to CalFed over the years, leads me to believe that the state of knowledge is poor. I am sure that generic models can be modified to suit the system but it seems to me that good, hard data are needed more than models at this point. If I am wrong, then at least I think the authors needs to explain more clearly the relationship between the models and the existing data (and gaps therein).
Rating	good

## Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	This project does not have a major monitoring compoment.
Rating	not applicable

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The models produced are likely to help identify, in general terms, some of the important areas of uncertainty, and also some of the relationships between physical and life history variables.
Rating	very good

## Additional Comments

### Comments

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors have a sophisticated team of modelers, and their record of publications is sound. As indicated above, I am concerned about the weaknesses in terms of salmon biology, and the appreciation for the value of good data. Statements such as "enormous quantified of data have been collected ... and we therefore propose ... a thorough exploratory analysis of the data" seem vague and naive to me.
Rating	very good

## Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Given the absence of field work, the overall budget seems high. The staff are virtually all at high levels, so the costs per unit time are high. These people are experienced so the salaries themselves are not unreasonable but the result is a high cost.
Rating	good

## Overall

Provide a brief explanation of your summary rating.

Comments	As indicated above, the need for models is clear, and they have real value. I do not mean to be overly harsh in my assessment, as these are highly qualified
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Technical Review #2

	scientists. However, I think that the lack of clear integration with empirical information (or lack thereof) weakens this proposal.
Rating	good

# Technical Review #3

proposal title: MODEL–BASED GUIDANCE FOR RESTORING CHINOOK SALMON: RIVER, ESTUARY, AND OCEAN INFLUENCES ON POPULATIONS SPAWNING IN THE SACRAMENTO AND SAN JOAQUIN BASINS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The proposed project aims to develop different models to address questions about the relative role of three threats to Chinook Salmon populations: tributary flows into the estuary system, temperature constraints on local fish abundances, and the importance of ocean survival. The proposal is loosely prepared with inconsistent presentations of the goal and objectives, and more pages of text than allowed. Some attention will be directed to steelhead trout in parts of the work. Both species are important to the CALFED program because of their poor status and importance to the public. The application of models for some questions about salmon population constraints will likely yield some new information on the species.
Rating	good

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The proposed set of models and species limitations appear relevant to current water management decisions. Ocean survival is a basic issue when considering what
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### Technical Review #3

	inland management actions might achieve for species conservation. A clear basis for investing effort on a partial set of population threats with distinct modeling efforts is not given in a convincing manner. The conceptual mode illustration does not convey a logic for what was included in the plan. I must conclude then that this project would address some prominent questions about conservation of chinook salmon but not in a coordinated manner that would allow decision makers to balance tradeoffs of water uses and benefits for the species.
Rating	good

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The designs of modeling tasks have some weaknesses. The selection of two tributaries with different thermal regimes can yield contrasting results but why this design is best for understanding or management relevance was not clearly provided. Habitat at the meso scale is to be addressed but not enough details are given to judge its viability. Ocean survival modeling is addressed in just a very general way. Potential fish migration routes through the delta are expected to be influenced by river flows but the modeling methods to do this are not adequately communicated. Therefore, it seems the modeling could provide some new and important findings but I expect they would be fragments of information not easily used in a system wide context or for recognizing tradeoffs in decision making.
Rating	good

## Technical Review #3

### Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?  
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	As note above I felt there were weaknesses in the plan that raised concerns for me on feasibility and effectiveness of the models. On the scale of species conservation, I feel this project would have limited utility because it is a fragmented set of modeling efforts. Little is given on how the different modeling efforts and results would be synthesized for application. Little effort is described for doing that too: some meetings. Overall, the proposed project lacks a common vision of purpose and conceptual basis for integration of some distinct tasks.
Rating	good

### Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Population monitoring is not directly addressed. If effective, one or maybe two of the models would yield findings usable in monitoring program design and execution.
Rating	fair

### Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	
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### Technical Review #3

	Addressed in above comments - this project might be best viewed as a set of some relevant modeling tasks that could contribute new information. The actual products are not well described. A decision tool is promised but I could not visualize what that would actually look like. Most likely I think is the current Oak Ridge Chinook Model would be enhanced. I am not sure the extent that the current model is used by decision makers or species managers.
Rating	good

## Additional Comments

### Comments

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The investigators named under different tasks have a record of producing models and results from them. Each has different expertise that is relevant to the proposed project needs. There seems to be little record of pooling models together for a unified species analysis, and not much effort is devoted to doing that in the project plan.
Rating	good

## Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This seems like an expensive project because it is largely a modeling effort. Some field data is to be collected but that is not described enough to judge its cost and scope.
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### Technical Review #3

<b>Rating</b>	good
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## Overall

Provide a brief explanation of your summary rating.

<b>Comments</b>	The project appears to me to be a poorly planned set of modeling efforts. The aims and objectives vary through the proposal. Sources of data are not clearly given. Steelhead trout are sometimes included in the tasks and described as a preliminary aspect of the effort. Overall I am concerned this is too loosely developed and if funded would results in a mix of activities with partial success.
<b>Rating</b>	good